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THE FATAL ILLNESS OF NAPOLEON

A PAPER READ BEFORE THE HISTORICAL SECTION OF
THE INTERNATIONAL CONGRESS OF MEDICINE
LONDON
(*August* 8, 1913)

BY

ARNOLD CHAPLIN, M.D. (CANTAB)

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, LONDON

PRINTED FOR THE AUTHOR
FOR PRIVATE CIRCULATION

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Professor Arthur Keith F.R.S.
With the Author's Kind regards.

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The extraordinary interest which the subject of Napoleon arouses at the present day was one of the reasons which prompted me to offer for your consideration a paper on the fatal illness of the great Emperor. Another was that the International Congress of Medicine appeared to afford an excellent opportunity for obtaining a medical verdict of a representative and international character on the nature of the illness, —a verdict which would command the respect and attention of all students of the Napoleonic period.

I would, therefore, respectfully ask the members of the Historical Section here assembled to consider themselves in the position of a tribunal or commission called together to decide the following questions :—

1. What were the diseases from which Napoleon suffered during his detention on the Island of St. Helena ?

2. What were the probable causes of those maladies?

3. How far did the results of the post-mortem examination substantiate the clinical evidence of those diseases?

If you will accept the position of judges in this inquiry, and will accord me the permission to place the facts before you, I will endeavour to perform my task without appealing to your prejudices or sentiments in any way.

During the discussion which I trust will spring from this paper, gentlemen will, doubtless, address us who may be regarded as special pleaders in the cause of some particular disease from which it has been asserted the Emperor suffered. Indeed, I think no student of the subject can have failed to notice of late a somewhat alarming increase of the maladies which are reputed to have attacked Napoleon, and I would even venture to make an appeal, on behalf of the Emperor, to those who would multiply his illnesses, and ask them not to assign to him more of the ills than the flesh can be reasonably expected to inherit.

In presenting the case to you, unfortunately I have no witnesses to call, for Napoleon has been dead ninety-two and a quarter years, and all the evidence I can offer has been taken, so

to speak, on commission. However much you might desire to cross-examine the doctors in attendance, and in some cases that would be most interesting, yet it is impossible, and you must be content to take their recorded statements, and arrive at a conclusion from a consideration of them alone.

What is the nature of the medical evidence we possess of the illness of Napoleon while on the Island of St. Helena? It has come down to us in the form of books or reports furnished by eight medical men, viz., O'Meara, Stokoe, Antommarchi, Arnott, Shortt, Henry, Rutledge, and Burton. The four first-named attended the Emperor during the various phases of his malady or maladies, while the last four were present at the post-mortem examination only, and can, therefore, speak of the pathological conditions found at the autopsy alone. O'Meara, Antommarchi, and Arnott have left behind them complete books dealing with the subject, while the statements of Stokoe, Henry, Rutledge, and Burton exist in the *Lowe Papers*, vols. 20,133, 20,157, 20,214; and the brief account from the pen of Shortt is found among his unpublished papers. Besides these sources of information, the *Lowe Papers* contain in vols. 20,156, and 20,157, the original reports of O'Meara, Stokoe, and Arnott, and as the books of O'Meara and

Arnott were published after the death of Napoleon, these statements are of much value as a means of checking the facts in the published works.

I must also ask you to remember that the mirror of truth has had its brightness considerably dimmed by the action of the violent passions and prejudices, both political and partisan, which filled the minds of those who were in close proximity to Napoleon in St. Helena. At this distance of time it is difficult to realize adequately the effect produced by these conflicting views in obscuring the proper proportions of the illness of the Emperor. Though stripped of all earthly power, the magnetic personality of Napoleon was still the storm centre, and in the narrow circle of his prison home, the minds of men were moulded to his imperious will as they had been when Europe was at his feet. In St. Helena truth was liable to be distorted by political intrigue. At every turn the policy of Longwood was matched against that of Plantation House, and anything likely to further the aspirations of either party was adopted with eagerness.

Briefly stated, the policy of Longwood sought to establish the fact that the detention of Napoleon was a long-drawn-out agony, while the British authorities attempted to prove with no less insistence that the captivity was running its course in a pleasant manner, and that everything

was being done to mitigate the rigours of confinement, compatible with the safe custody of the prisoner. The party at Longwood proclaimed to the world that the climate of St. Helena, added to the harsh treatment meted out to the Emperor, had produced endemic hepatitis which was gradually killing him. This charge was met by the British authorities with a flat denial. They insisted that the illness was diagnosed wrongly, and greatly exaggerated, if, indeed, it existed at all. They refused to believe in the theory of endemic hepatitis, since acquiescence would damage their contention that St. Helena was an ideal place in which to confine a fallen Napoleon. As usual, the truth lay between these two extremes.

It is not surprising, therefore, that the canker of exaggeration and violent partisanship ate its way into the deliberations of the doctors, vitiated their views, and divided them, after the manner of their superiors, into two hostile camps, of which one was representative of the policy of Longwood and the hepatitis theory, while the other was for the British authorities and its practical negation of ill health. In this difference of opinion O'Meara, Stokoe, and Antommarchi represented the Longwood policy of Napoleon and his followers, while Arnott, Baxter, Shortt, Henry, Rutledge, and Burton were

responsible for upholding the views of the British authorities.

Now if the account just given of the political condition existing in St. Helena be true, it follows of necessity that all the evidence we have must be treated as suspect, and must be subjected to a rigorous investigation, if we are to arrive at a conclusion free from prejudice and error. The problem of deciding the nature of the illness of Napoleon will be solved with greater ease if we confine ourselves to a consideration of the clinical evidence first, and leave the pathological facts until the end of the paper.

What clinical evidence have we on which to form an opinion? From the day Napoleon left Plymouth Roads to July 25, 1818, all the medical evidence we possess proceeds from the able pen of Barry Edward O'Meara. During that period no other medical man saw Napoleon professionally, for the visits of Dr. Baxter were those of courtesy only. The evidence of O'Meara is found in his famous *Voice from St. Helena*, published after Napoleon's death, and in a series of reports of the condition of the Emperor's health, addressed every week to Sir Hudson Lowe. These reports fill the greater part of vol. 20,156 of the *Lowe Papers*. Of the two sources of information, the health reports written on the spot must, I think, be regarded as of greater

value than the evidence contained in the *Voice from St. Helena*, which was written for the public as a complete vindication of O'Meara's conduct, after the death of Napoleon had put an end, in large measure, to the storm and strife which surrounded him. Then I must ask you to remember that O'Meara has been accused of being sadly wanting in historical accuracy. Long before he left St. Helena he became the most uncompromising and vindictive opponent of the British authorities and their policy as directed by Sir Hudson Lowe, and many experts refuse to give credence to his statements unless supported by corroborative testimony. But in this matter of medical evidence it is not contended that O'Meara has deliberately falsified facts; all that is asked is that the examination of his evidence shall proceed with caution, on account of his reputation as a witness and his avowed animosity to Sir Hudson Lowe and his policy. For these reasons it will be necessary to check O'Meara's facts, and to compare the evidence in the *Voice from St. Helena* with his original health reports in the *Lowe Papers*.

After O'Meara's departure from St. Helena at the end of July 1818, no medical man saw Napoleon professionally until January 17, 1819, when Mr. Stokoe, the surgeon of H.M.S. *Conqueror*, was hastily summoned to attend Napoleon, who

had been suddenly seized with an attack of vertigo and syncope. Stokoe saw Napoleon five times, and then his visits were ordered to cease. An excellent account of his brief association with Napoleon can be read in *Napoléon Prisonnier*, by M. Paul Frémeaux. Stokoe's testimony is not open to the charge of want of veracity, and may be accepted as a true account of his view of the illness.

After the enforced retirement of Stokoe from the case, Napoleon was again left without any professional assistance until September 23, 1819, when Francesco Antommarchi, who had been sent out to St. Helena as physician to the Emperor, paid his first visit to the patient. Antommarchi remained with Napoleon to the end, and during the last thirty-five days of the illness, he was associated in the treatment with Dr. Archibald Arnott, the surgeon to the 20th Regiment. The evidence of Antommarchi is contained in his well-known book, *Les Derniers Moments de Napoléon*, published in 1825, and therefore after the death of the Emperor. In the examination of the testimony of Antommarchi, the greatest caution and reserve must be exercised, for what has just been said about O'Meara, applies with added force with respect to Antommarchi. The book abounds with errors and false statements, and there can be no doubt

that many of them were made by design. Unfortunately, the parts of his diary which deal with Napoleon's illness cannot be submitted to the test of any collateral evidence, for no other medical man saw the patient, if we except Arnott's attendance during the last thirty-five days, but where he deals with matters within the knowledge of other witnesses his mendacity is only too apparent. Indeed, of all the first-hand evidence relating to the captivity, Antommarchi's is the least reliable, and must, therefore, be scrutinized with the greatest care.

Arnott published in 1822 a pamphlet giving an account of the last thirty-five days of the Emperor's illness; and the opinions he formed during that period are recorded day by day in vol. 20,157 of the *Lowe Papers*. The two accounts differ materially, and, of course, more weight will be attached to the reports made at the time than to the considered statements in the pamphlet written after the death of Napoleon.

Having then briefly indicated the nature of the medical testimony at your disposal, and having drawn your attention to the question of the veracity of some of it, I propose now to lay before you an account of the illness of the Emperor as it is disclosed in the records available.

The first evidence I offer for your consideration is that of Barry O'Meara, who landed in

St. Helena with Napoleon on October 17, 1815, and remained in professional attendance until July 25, 1818. We will take the evidence in his book, the *Voice from St. Helena*, first, and deal with the health reports in the *Lowe Papers* afterwards. The references to ill health in the *Voice* may be divided into two periods; one dating from October 17, 1815, to September 30, 1817, and the other from October 1, 1817, to July 25, 1818. During the first period, O'Meara makes some thirty-seven allusions to Napoleon's ill health, but he does not mention indisposition until May 14, 1816, so it may be assumed that Napoleon was well until that date. All the references in this period deal with ailments which may be described with fairness as "trifling"; and although these attacks sometimes lasted for a few days, they were never so serious that they altered the Emperor's particular mode of living at that time.

During this first period what was the nature of these attacks? The first one noticed by O'Meara was catarrhal, produced, he says, by walking out in thin shoes in the wet. Then attacks of headache of a nervous type are recorded on about fourteen occasions, and both Napoleon and O'Meara attributed them to want of exercise, for, at this time, the prisoner maintained a strict seclusion, and often did not go out of the

house for weeks at a time. On July 26, 1816, there is a note of a slight pain in the right side, for which O'Meara recommended rubbing. On October 1, of the same year, carious teeth began to give trouble, and on the 23rd one cheek became swollen, and painful, and the gums were spongy. In this condition the Emperor went out for a drive, the first one for six weeks, and on his return at 5 p.m. he was seized with shiverings and rigors, which O'Meara found to be due to an attack of tonsillitis, and it was not until November 7 that the surgeon could pronounce his patient well.

The months of December 1816 and January and February of 1817 contained no reference to ill health, with the exception of attacks of headache, but on March 24, slight swelling of the legs is recorded, a condition which had been remarked once before in the preceding November. April and May produced nothing of importance, but towards the end of that month the state of the teeth again produced inflammatory trouble in the right cheek, and these symptoms recurred in June and September. In the middle of September 1817, O'Meara informed Lowe that, with the exception of slight catarrhal attacks, Napoleon's health had been tolerable, and that his illness was not of a serious nature.

So for the first period there is very little in

O'Meara's records to point with any certainty to the inception of a serious malady. If this be true of the first period, it is, however, equally certain that the second, which began at the end of September 1817, was ushered in by symptoms which gave great cause for uneasiness in the mind of O'Meara, for on September 30, 1817, Napoleon, who had been far from well for some days, was attacked with illness, the symptoms of which were quite definite, and persisted with but slight intermission to the end of O'Meara's stay on the island.

These definite symptoms may be summarized. They were—(1) a dull pain in the right hypochondriac region; (2) a sensation akin to numbness in the right scapular region; (3) a pulse of 68; (4) spongy gums; (5) nausea; (6) a slight cough; (7) feverish attacks ending in abundant sudoresis; (8) headache; (9) palpitation; (10) œdema of the legs; (11) general weakness. O'Meara examined his patient on October 3, and stated that the right side was firmer to the touch than the left, and that there was a tumefaction evident to the sight in that region, which when pressed gave a sensation of pain. The diagnosis was, that if these symptoms increased there would be no doubt that the disease was hepatitis.

From October 1, 1817, to July 25, 1818, the date on which O'Meara last saw Napoleon, these

symptoms continued with varying intensity, and they were never entirely absent. During all the time the Emperor's health was bad, he showed more and more disinclination to exert himself, and when recommended to take exercise, obstinately sheltered himself behind the excuse that the restriction of limits imposed upon him rendered such an action impossible. O'Meara saw Napoleon for the last time on July 25, 1818, and reported that he found him in much the same condition of ill health that had become habitual since the onset of the symptoms noted above.

In considering these statements made in the *Voice from St. Helena*, I must draw your attention to the fact that up to the end of the first period of ill health, O'Meara was on fairly good terms with Sir Hudson Lowe; but by the time the graver symptoms made their appearance, the quarrel which had been brewing between Lowe and O'Meara became acute, and I think it may be fairly urged that the tone of his narrative becomes more coloured with his enmity towards Lowe just at the time when Napoleon became seriously unwell. Indeed, the last 150 pages of the second volume of the *Voice*, which deal with the second phase of Napoleon's illness, are so unmistakably devoted to an attack upon Lowe, that their value as an unvarnished medical testimony is largely destroyed.

Having given in outline the main features of O'Meara's account of Napoleon's illness as set forth in the *Voice from St. Helena*, let me direct your attention to his health reports written from week to week, and preserved in the *Lowe Papers*, vol. 20,156. These reports are simple statements of the symptoms and progress of Napoleon's malady. Unlike the *Voice from St. Helena*, they are not designed to justify O'Meara's conduct, but were merely written to acquaint Sir Hudson Lowe with the state of the health of his prisoner. Therefore, because they are unmixed with contentious matter, they are more reliable than the reports in O'Meara's book.

The first report is dated October 20, 1816, that is five months after Napoleon had been in indifferent health, according to the *Voice*, and they continue every week with but few intermissions until October 9, 1817. But at this time difficulties arose concerning the reports, and Napoleon refused to see O'Meara professionally so long as he made written reports to Sir Hudson. A compromise was, however, effected by Lowe agreeing to accept reports from Dr. Baxter, the principal Medical Officer, on the understanding that they were based on the verbal reports of O'Meara. Thereafter, until the departure of O'Meara, the reports were written by Dr. Baxter.

In these reports the record of symptoms is

practically the same as that found in the *Voice from St. Helena*, but it can be stated with truth that the causes of the indisposition and the deductions made differ somewhat from those detailed in the book. All through the health reports O'Meara is careful to insist that Napoleon's mode of life is largely responsible for his ill health; for instance, he speaks of "total lack of exercise," of Napoleon's being "closely shut up in his room with windows fastened for days and even weeks," and of the refractory nature of the patient. In one report he says: "By timely measures, I have no doubt he would soon be restored to health, but he will not do as he is advised." Then the *Voice from St. Helena* mentions frequent nervous headaches, but the health reports tell us that Napoleon informed O'Meara that they were nothing new, for he had suffered from them frequently for some years before he came to St. Helena. Both reports mention the œdema of the legs, but Gourgaud is responsible for the statement that this had been a condition with Napoleon ever since the Russian campaign. Then I think it may be said that the account of the febrile manifestations, attended with abundant sweating, loses some of its intensity in the health reports. Lastly, there is Gourgaud's statement to Mr. Goulburn in London, in which he asserted that

up to the time of his departure from St. Helena, in January 1818, Napoleon's bodily health had not been worse than it had been for some time previous to his arrival in the Island, and he based this statement on an intimate knowledge of the Emperor's habits of life.

But there are still other documents in existence from the pen of O'Meara which throw some light on the question. While he was compiling his journal and while he was writing his official weekly reports of Napoleon's health, he was busily engaged in a correspondence with his friend Mr. John Finlaison, the keeper of the records of the Admiralty. Copies of these letters fill two volumes of the *Lowe Papers*, and they exhibit O'Meara as a delightful correspondent, whose chief aim was to tell his friend everything of interest that was taking place at Longwood. So interesting are these letters that it is astonishing that no editor has ever considered their publication, for without doubt they would enhance O'Meara's reputation as a writer. In the Finlaison letters, he does not say a great deal concerning Napoleon's illness, and when he does, he attributes it rather to his invincible determination to live a life devoid of exercise, and calculated to break most of the ordinary laws of health.

When O'Meara left St. Helena, Dr. Verling,

the surgeon to the Royal Artillery, and a man of considerable education, was appointed, by Lowe, physician to Longwood. But the appointment was a complete sinecure, and need never have been made, for Napoleon adhered to his refusal to receive him, and during the whole time he was in residence Verling had no professional intercourse with the Emperor.

Before the departure of O'Meara, Drs. Baxter and Verling requested him to show those parts of his journal which related to Napoleon's health. But this he flatly refused on the grounds of professional etiquette. He, however, volunteered to make out an account of the case for the use of Verling. Those parts of the diary which deal with the illness may exist among O'Meara's papers in America, just as they appear in the *Voice from St. Helena*, but they have never seen the light, and Forsyth and others have even doubted whether they exist at all. Indeed, in the editorial commentary attached to the unpublished portions of O'Meara's original diary, which appeared in the *Century Magazine*, February–April 1900, we are told that the manuscript ceases in the early part of 1817, in fact, before Napoleon became seriously ill. The proceeding was rather suspicious, for O'Meara and Verling were on fairly friendly terms.

From July 25, 1818, to January 17, 1819,

Napoleon was not seen by any medical man, he was in very strict seclusion, and hardly ever went out of doors. On January 17, Dr. John Stokoe was hastily summoned to the Emperor, who had been seized with an attack of vertigo which culminated in unconsciousness. Stokoe tells us that he found Napoleon suffering from very much the same symptoms that O'Meara had described, so it is reasonable to infer that very little change had taken place in his condition. Stokoe's visits were ordered to cease in three days, and his statements regarding the symptoms of the illness were, most unjustly, treated as untrue, or at all events as exaggerated. The testimony of Stokoe is open to one objection only. O'Meara had been on friendly terms with him, had introduced him to Napoleon, and had extracted a half promise from the Emperor that he would call in Stokoe, if in need of medical assistance. He was weak and pliable, and it is within the realm of probability that O'Meara's views became grafted on to those of Stokoe.

After the compulsory retirement of Stokoe, Napoleon was again without the assistance of a doctor, until Antommarchi paid his first professional visit on September 23, 1819. He deals more fully than any other medical writer with the symptoms and progress of the Napoleonic malady, and one could wish that the statements in his

book were less open to the flat accusation of untruthfulness. But from first to last the book bristles with inaccuracies, and of all the testimonies of eye-witnesses of the St. Helena period, is perhaps the only one which has failed to find an authority who would accept without proof the statements contained therein. This is much to be regretted, for Antommarchi was an accomplished anatomist and a skilled pathologist, and could his word be relied on his testimony would be of inestimable value in solving the riddle of the Emperor's disease.

Before Antommarchi sailed for St. Helena he was in close touch with O'Meara, and became fully acquainted with his views regarding Napoleon's malady.

At his first professional visit, on September 23, 1819, Antommarchi made a careful examination and found the Emperor with a coated tongue, a pulse of 60, a dry cough attended with viscid expectoration, and on palpation he found the region over the left lobe of the liver very tender. Napoleon told him that he suffered from more or less constant dull pain in the right hypochondrium, and a pain in the right breast and shoulder. He also complained of nausea, the vomiting of bitter bilious matters, and nightly profuse perspirations. In fact the symptoms and signs were much the same as those described by O'Meara,

and the Emperor, confined as he had been to his narrow and stuffy rooms for two years with frequent recourse to hot baths, presented a sad spectacle of enervated health and flabbiness of fibre.

Antommarchi at once set himself the difficult task of attempting to break down the invincible repugnance of his patient to take exercise in the open air. At first he was unsuccessful, but in a few days Napoleon consented to take his advice, and during the month of October 1819 he was out nearly every day. He was in a most feeble state, but little by little the effect of the fresh air began to show itself, and by the end of the month Antommarchi was able to describe Napoleon as well. The symptoms returned, however, on November 11, and lasted a week, and another relapse occurred on December 17, but was recovered from on December 21. The intervals between the attacks were becoming longer, and the duration of the seizures shorter, and after this no further attack took place until July 20, 1820, an interval of seven months. This was the time when Napoleon took up gardening, when he was well and out of doors every day, and was apparently free from cares and vexations.

Antommarchi had done an immense service to Napoleon, he had succeeded where O'Meara had failed, and had persuaded the Emperor

to live a reasonably healthy life. The result was a large measure of improved health. Now it is a peculiar fact that during this period from September 23, 1819, to July 20, 1820, Antommarchi, when describing Napoleon's symptoms, makes no mention, except on October 24, of those of fever which were such a marked feature in O'Meara's reports. He does not mention the abundant perspirations until July 20, nor does he draw attention to the increased heat of the body, the rapidity of the pulse, and the shivering fits. He, however, speaks much of the headache, and the abdominal discomfort. It may, therefore, be inferred that these symptoms were not prominent during this period.

The attack in July lasted for about ten days, and there is little to record until September 18, 1820, when Napoleon again became unwell. About this time symptoms began to make their appearance which pointed unmistakably to serious disease in the alimentary tract. Indeed, the character of the illness completely changed and, from this date to the end, the case became one of comparative simplicity. It is true there were intervals in which all the symptoms lost a considerable part of their intensity, but the sum total of Napoleon's condition during the last period of his illness was steady progression to a fatal termination.

What were these definite symptoms which stamped the illness as one belonging especially to the alimentary tract? The most persistent one was vomiting, and this condition was never absent for many days. Next to that in frequency came a disordered state of the bowels. Sometimes there was constipation and sometimes diarrhœa. Then there was considerable abdominal discomfort, evidenced by gaseous distension, colic, heaviness, and pain in various regions of the abdomen, but most commonly situated in the epigastrium and the right hypochondrium. Added to these manifestations were a steady progressive weakness, icy coldness of the extremities, and, from time to time, exacerbations of fever which always ended in abundant sweating. These were the most prominent symptoms during the last seven months of Napoleon's existence. As time went on all of them became more and more pronounced, and the vomiting especially became incessant. The fever also rose in intensity, and the weakness and wasting made rapid strides. Under the weight of these grave phenomena Napoleon's strength gradually sank. By the end of March 1821 the case was hopeless, but he lingered on, becoming steadily worse, until May 5, 1821, when he died at eleven minutes to six in the evening.

On April 1, Dr. Arnott, the surgeon to the 20th

Regiment, was called in, and remained in attendance until the end. At first he was sceptical as to the serious nature of the illness, and was inclined to the opinion that most of the symptoms pointed to hypochondriasis, but towards the end he saw reason to alter his view, and when, on April 27, the matters thrown off the stomach were seen to be of "coffee ground" consistence, he no longer doubted that serious disease of the stomach was at the root of Napoleon's indisposition.

It may be stated, therefore, that, soon after the middle of September 1820, the illness of the Emperor took on a new phase, and that, from that time, the symptoms pointed to the onset of grave gastric disease, very different from the indisposition from which he had suffered for the previous three years. This change in the symptoms may, I think, be fairly attributed to the beginning of the cancer of the stomach which eventually caused his death. It was one of those rapid-growing carcinomas which, when they attack the body of the stomach, generally result in a fatal termination in six or eight months.

The post-mortem examination of Napoleon now claims your investigation. This took place on May 6, in the drawing-room at Longwood, a little after 2 p.m. It was not a prolonged examination, for Sir Thomas Reade, who repre-

sented Sir Hudson Lowe, wrote to him on that day, and dated his latter 4 p.m. In the letter he informs Lowe that the examination is finished, and that Dr. Shortt has already left Longwood to give Sir Hudson a verbal account of the proceedings. Therefore, allowing for the time it would take to sew up the body, a proceeding Sir Thomas required to be done before he left the room, the examination lasted about an hour and a half, and was performed in daylight.

Seven British doctors were present, viz.: Shortt, Arnott, Burton, Mitchell, Livingstone, Henry, and Rutledge; and Antommarchi was the operator. Three accounts of the results of the examination are in existence—the official one, Antommarchi's, and Henry's—while Rutledge and Shortt have left brief statements of the appearances noticed. Antommarchi's is without doubt the fullest and the best, and proves the reputation he claimed as a pathologist. It will not be necessary to quote in detail the results of the examination, for they are well known, and can be read in Antommarchi's book and in the *Lowe Papers*, but attention will be confined to the chief appearances exhibited, and to the points wherein the accounts differ.

In one important particular all the reports are in complete agreement, and that is the stomach. This organ was found to be the seat of extensive

carcinoma in a state of ulceration which, with the exceptions of about an inch around the cardiac orifice and a small portion along the greater curvature, involved practically the whole organ. Then all accounts agree in stating that adhesions existed uniting the stomach along its lesser curvature to the concave under-surface of the left lobe of the liver. There is no dispute concerning these facts, and they may be taken as true.

When, however, the question of the state of the liver and its capsule comes under consideration, we plunge at once into contradiction, obscurity, and doubt. But this can be only expected, for the state of the liver would decide once and for all time whether the contentions of the Longwood household or those of the British authorities were to prevail.

What have the doctors who were present at the post-mortem examination told us about the state of the liver and its capsule? Antommarchi informs us in his book that "the spleen, and the liver which was hardened, were very large and distended with blood. The tissue of the liver, which was reddy-brown in colour, did not, however, present any other notable alteration in structure. The liver, which was affected with chronic hepatitis, was closely united by its convex surface to the diaphragm; the adhesion extended over the whole organ, and

was strong, cellular, and of long standing." The seeming contradiction between the statement that "the liver did not present any notable alteration in structure," and that "it was affected with chronic hepatitis," will be noticed. Then at the autopsy, Antommarchi, when he had cut into the liver, remarked to Sir Thomas Reade: "It is good, perfectly sound, and nothing extraordinary about it except that it is a large liver." Again, when the official report was read over to him by Shortt and Burton, Antommarchi expressed himself in complete agreement with its findings. These discrepancies must, therefore, be remembered when assessing the value of Antommarchi's testimony.

All the other doctors present, with the exception of Dr. Shortt, asserted that the liver was normal in size and structure. Shortt, however, thought the liver was enlarged, although he was satisfied as to its soundness.

The adhesion of the convex surface of the liver to the diaphragm is also a subject which gave rise to much divergence of opinion. Antommarchi, as we have seen, said it extended over the whole organ, and was strong, cellular, and of long standing. If this be a correct description, it is of course strong evidence of inflammatory trouble having attacked the liver at some time or other. But the official report says the adhesion was between

the convex surface of the left lobe only and the diaphragm; and adds, "with the exception of the adhesions occasioned by the disease of the stomach, no unhealthy appearance presented itself in the liver." Henry also, in his account, speaks of a small adhesion to the surface of the left lobe of the liver, which appeared to be a continuation and a consequence of the adjoining adhesions between the liver and the stomach. But the most direct contradiction of Antommarchi's statement concerning the adhesions between the diaphragm and the liver comes from Rutledge, who wrote a letter soon after the appearance of Antommarchi's book. Rutledge quotes Antommarchi's statement, and then says: "There was no adhesion between the liver and the diaphragm, except through the medium of a little coagulable lymph, which I easily removed with my finger when taking out the liver for examination."

This is all very perplexing, and in order that you may compare with greater ease the various statements made by different witnesses concerning the liver, I have placed the evidence together, and must leave you to draw your conclusions therefrom (see Appendix I).

There are several other points in the results of the post-mortem examination which require notice. Antommarchi mentions that he found

the peritoneal membrane lined with a soft, diffluent transparent exudation. He also says, "I observed on the peritoneal surface of the intestines and in its folds small spots and patches of a very light red colour, of various sizes, and disseminated. The mucous membrane of the digestive canal appeared to be in a healthy state." Henry's report and the official document say that the intestines were sound. Then Antommarchi mentions the presence of tubercles and some small tuberculous excavations at the apex of the left lung, and he also describes the lymphatic glands of the small omentum as being tumefied and scirrhous, and some in a state of suppuration. The bronchial glands, and those of the mediastinum, were found by Antommarchi to be slightly enlarged, almost degenerated, and in a state of suppuration. But the condition of the bronchial glands, the lymphatic glands of the omentum, and the presence of obsolete tuberculous excavations at the apex of the left lung, are not mentioned by any of the other doctors who attended the post-mortem examination, and therefore rest on the assertion of Antommarchi alone. Finally, it remains to be said that Henry took notes of the appearances exhibited during the progress of the examination, and Dr. Graves of Dublin has stated that his cousin, Dr. Burton, did the same. These notes

of Dr. Burton are at present undiscoverable, but from what Sir Thomas Reade has told us in his report, we may assume that they did not favour the hepatitis theory. Whether Antommarchi took notes as he performed the examination is unknown, but having in view the fact that he was the operator, and that the examination did not last much longer than an hour and a half, it may be doubted if he could have had time to do both.

An account has now been given of the symptoms of Napoleon's illness, and of the appearances found at the post-mortem examination, and it becomes our duty to consider the deductions which have been drawn by various authorities from these facts. All are I think agreed that the ultimate cause of death was cancer of the stomach. Indeed, with the uncontradicted statement before us, attested by all present at the post-mortem examination, that nearly the whole of the stomach was in a state of ulceration, and in view of the undisputed facts that for weeks before his death, Napoleon had been unable to retain anything for long on his stomach, suffered from incessant vomiting, and presented other symptoms of serious disease of the stomach, no other conclusion appears possible. Surely the fact that nearly the whole of the internal surface of the stomach was converted into a huge

cancerous ulcer may be regarded as sufficient to cause the death of a man.

It is true that Héreau, in 1829, and later Boudoin, have sought to prove that the supposed cancer was nothing more than an inflammatory condition, but I have the weight of the authority of Professor Arthur Keith, who tells me that no one who reads over Antommarchi's careful description of the post-mortem appearances of the stomach could come to any other conclusion than that the disease of the stomach was cancer. In support of that statement he quoted the opinion of the eminent pathologist Mr. Shattock who, after reading over Antommarchi's account, came to the conclusion that the disease of the stomach there described was cancer and cancer alone. Indeed, higher opinions than these would be difficult to find. I mention this particularly because I have heard it stated that Professor Keith does not believe that Napoleon had cancer of the stomach.

But if there appears to be little doubt that the ultimate cause of Napoleon's death was cancer of the stomach, there are still the symptoms from which he suffered during life to be considered, some of which are not in accordance with those of gastric cancer. Indeed, as has already been pointed out, there is considerable difficulty in regarding the symptoms of the illness during

the whole period of Napoleon's ill health as due to cancer alone. For, on that supposition it would make the duration of the cancer far too long, and it was only some eight months before death that definite symptoms pointing to gastric cancer began to make their appearance. Many explanations have been offered, to account for these discrepancies, the most usual being hepatitis, and having regard to the symptoms during the earlier part of the illness, the diagnosis is by no means unreasonable. But if hepatitis existed for three and a half years, surely undoubted evidence of that condition would have been disclosed at the post-mortem examination, either in the liver itself or its capsule. But, as I have pointed out to you, there is a direct conflict of evidence on this point. Antommarchi alone describes the liver as being affected with chronic hepatitis, although, in a sentence just before, he states that it exhibited no alteration in structure. Antommarchi alone tells us that the adhesions between the liver and the diaphragm extended over the whole organ, and were strong, cellular, and of long existence. All the others present at the post-mortem examination assert that the liver was sound, and that a small adhesion only was found between the liver and the diaphragm, while Rutledge roundly accuses Antommarchi of mendacity, and affirms that

there was an adhesion between the liver and the diaphragm produced only through the medium of a little coagulable lymph, which he easily removed with his finger. Then there is the letter of Dr. Burton, in which he says that Antommarchi expressed agreement with the official report, but refused to sign it on the advice of Count Bertrand.

In deciding the actual state of the liver and its capsule, the credibility of the witnesses is all-important. Do you believe Antommarchi, the undoubtedly capable pathologist, the man who published the anatomical plates of his master Mascagni as his own; the man who stole the mask of Napoleon from the owner, Dr. Burton (who executed the work after Antommarchi had failed), and then asked the world to believe that it was his and his alone; the man who, in his book, inserts long conversations with Napoleon, on days when it was known that the Emperor refused to allow him in his presence; and the man whose book, beyond all others dealing with the period, has been found sadly wanting when weighed in the balances of historical accuracy? Or, do you believe the seven British doctors who witnessed the post-mortem examination, men with no particular claim to pathological knowledge; men one and all fearful lest an expression of opinion contrary to the British

view might seriously compromise their positions, and destroy their chances of promotion; men dominated, it is true, by the narrow-minded policy of Sir Hudson Lowe, but also men against whom no charge of deliberate falsification of facts has ever been brought? Motives for stating certain facts will not help us, for both Antommarchi and the seven British doctors were biased, the former to support the climatic contentions of the Frenchmen, the latter in support of the British authorities with their negation of any climatic influences whatsoever. It is, in fact, Hume's contest between two opposite improbabilities, and must be decided by the method advocated by him.

Another theory, to account for the symptoms before the definite ones of gastric cancer, is that which attributes them to the presence of a chronic gastric ulcer. This theory has often been advanced, and has the support of Professor Ewald, Sir Lauder Brunton, and other authorities. There is much to be said for this view. The pain, its situation and character, together with other symptoms described during the course of the illness, make it a possible hypothesis. But this theory like all others does not account completely for all the symptoms noticed, yet it is one which deserves your attention, when considering the whole subject.

But another theory has been advanced lately to account for the symptoms from which Napoleon suffered during the first three years of his illness. Early this year a most important contribution to the study of this subject came from the able pen of Professor Arthur Keith. In a lecture which he delivered before the Hunterian Society, he propounded the view that Napoleon's indisposition was due to an endemic form of disease dependent upon particular climatic conditions present on the Island of St. Helena. He rested his thesis on two main premisses. The first premiss was based on his contention that two specimens of small intestine exhibited in the Museum of the Royal College of Surgeons, and described, "Incipient Fungus of the Glands of the Small Intestine, Napoleon, Barry O'Meara to Sir Astley Cooper," did, in fact, come from the body of the Emperor. On submitting these specimens to microscopic examination, Professor Keith found that the so-called Incipient Fungus was not cancer at all, but inflammatory in nature, and, indeed, what one would expect to find in a man who had been affected for a long period with chronic undulant fever.

His second premiss was based on the contention that during the three and a half years that Napoleon was ill, the symptoms exhibited corresponded in the main to those of undulant

fever, a condition which would have produced the appearances found in the specimens of the small intestines in the Museum of the Royal College of Surgeons.

In support of the first premiss, Dr. Keith contended :—

1. That Barry O'Meara obtained the specimens from Antommarchi, who surreptitiously abstracted them from the body of Napoleon, either during, or after, the post-mortem.

2. That Sir Astley Cooper was far too shrewd a man of the world to label these specimens as coming from the body of Napoleon, without satisfying himself that such was really the case.

3. That Antommarchi in his post-mortem report did incidentally describe the very appearances that these enlarged glands of the small intestine would exhibit when viewed from the external or peritoneal surface of the intestine; for he said: "In the peritoneal surface, and in the folds of peritoneum, I observed small spots and patches of a pale red colour, of various sizes and disseminated. The mucous membrane of this canal appeared to be in a sound state."

Now the evidence produced by Dr. Keith in support of his first contention is admittedly circumstantial in character, for there is no written evidence, and no verbal statement handed down,

so far as is at present known, which suggest that Antommarchi abstracted the specimens of small intestine from the body of Napoleon. The same may be said also of the contention that the specimens were handed to O'Meara by the Corsican pathologist. It is, in fact, a theory of probabilities, and must be decided by the usual methods adopted in such cases, of weighing the evidence for and against the theory, and rejecting that which is most improbable.

In order to enable you to arrive at a solution of this question, it will be necessary to state the evidence in existence which to a certain extent conflicts with the contention of Dr. Keith, and which he himself has stated with admirable clearness. In the first place, none of the reports of the post-mortem examination mention any apparent disease of the intestines, and Antommarchi in particular says that the intestines appeared to be healthy. In addition, in a letter to Count Montholon written before he published his book, he says the intestines were sound. Then his observation concerning the appearance of small pale red spots and patches in the peritoneal lining of the intestines may be read in conjunction with his statement that " a soft, diffluent and transparent exudation lined the whole extent of the contiguous part of the internal surface of the peritoneum," and the question may be

asked with fairness, whether those small pale red patches and spots may not have been patches of hyperæmia, which are by no means uncommon when peritonitis has been present.

Then there can be no doubt that the most rigid precautions were observed to prevent the abstraction of any parts from the cadaver. As already stated, the post-mortem examination was finished before 4 p.m., and therefore took place in broad daylight. The orders of Sir Hudson Lowe to Sir Thomas Reade, who represented him at the examination, were positive and explicit, to the effect that no abstraction of the parts was to be permitted, and that care was to be taken not to allow the cavities to be opened a second time. How did Reade perform his duty? In vol. 20,133, f. 133 of the *Lowe Papers* is to be found his exhaustive report of what took place at the post-mortem examination, and in it he describes minutely the precautions he took. After the examination was finished, he says: "I desired Dr. Shortt to give directions for the body being sewed up, and I requested it might be done previous to my leaving the room." This was done, and then Reade continues: "The heart was given in charge to Assistant-Surgeon Rutledge of the 20th Regiment, who was placed in charge of the corpse, and to whom I gave the most pointed orders that he was not to allow it

out of his sight." Seven British doctors and three combatant British officers, none of them very friendly to Antommarchi, the operator, were closely watching his movements, and two of the British doctors, Henry and Burton, were taking notes of the appearances exhibited as the examination proceeded. Rutledge, in vol. 20,133, f. 159 of the *Lowe Papers*, strongly dissents from Antommarchi's statement that the British doctors attended *ex officio*, and says: "From the very moment that the examination of the body commenced, all took a sufficiently active part to satisfy ourselves [*sic*] as to the nature of the disease." It does, therefore, seem that extraordinary precautions were taken to prevent the surreptitious abstraction of parts from the body of the dead Emperor.

After the post-mortem examination, the body of Napoleon was sewn up before Sir Thomas Reade left the room, it was then dressed and laid on the historic camp bed, with Assistant-Surgeon Rutledge in constant attendance all through the night of May 6. Rutledge has left a minute account of what took place during what he is pleased to call his "visitations at Longwood" (*Lowe Papers*, vol. 20,133, f. 150). He tells us how he watched the body all the night, and how on the following evening of May 7, at 7 p.m., he saw the body finally soldered up

in the coffin with the heart and stomach in separate vessels, and placed therein. Captain Crokat, the Orderly Officer, also shared this vigil, and during the whole day of May 7 marshalled those who filed past the dead Emperor. Doubts have been expressed by some as to whether Rutledge or Arnott was in charge of the corpse, but the information in the *Lowe Papers* leaves no doubt that Rutledge was the person in charge, and in addition there is the testimony of one Abraham Millington, the armourer, who did the actual soldering to the coffin. He tells us that Dr. Rutledge was the British surgeon in charge when he closed the coffin up (see *The Military Gazette*, March 3, 1838). There is also other evidence that extraordinary precautions were taken, for Professor Keith has published a letter from Sir A. R. Simpson in which is related Arnott's share in the proceedings. Arnott was apparently so fearful lest the heart and stomach should be stolen, that he took them into his bedroom on the night of the 6th of May, placed them in the basin, and retired for the night provided with two loaded pistols under his pillow. During the night he was disturbed by a noise; it was not Antommarchi or Montholon, however, but rats which were attempting to take away the imperial heart and stomach. It may also be asked why, if Antommarchi had

the specimens in his possession, he omitted to mention the fact in his book which was published several years after the death of Napoleon? He had nothing to fear, and the specimens, thought, as they then were, to exhibit secondary growths, would have strengthened further the contention he was concerned to prove, namely, that Napoleon died of a disease beyond the power of Antommarchi to cure. Again, attention must be paid to the reputations of the two main actors, in the transaction, Antommarchi and O'Meara, and the question must be asked: Can you believe O'Meara without corroborative testimony? On the other hand, you must also take into consideration the contention of Dr. Keith, that Sir Astley Cooper, who was well known for his knowledge of the world, would be the last person to be imposed upon by any one; and, indeed, one would expect that he would take steps to assure himself that the specimens were, as far as he could determine, genuine. In any case, however, he could have obtained no better evidence than the assertions to that effect of O'Meara, and possibly Antommarchi.

But Professor Keith's thesis does not rest alone on the authenticity of the specimens in the Museum of the Royal College of Surgeons. Even if they were rejected as spurious, there still remains his second premiss, which is based

on the contention that the clinical evidence strongly supports the theory that the symptoms exhibited by Napoleon during the course of his illness were in conformity with the view that he was suffering from an endemic form of fever produced by the climatic conditions existing in St. Helena. It is in this part of his argument that he has contributed so much to the study of the illness, and has thrown so much light on symptoms which were before obscure. It cannot be denied that some of the symptoms from which the Emperor suffered lend strong colour to the belief that some form of fever was present; and further, that these symptoms masked those of cancer, which was responsible for his death ultimately. It was towards the end of the illness that the symptoms of febrile disturbances became so prominent, for until January 1821 these manifestations were moderate in their intensity. It is, therefore, an interesting study to inquire into the nature of these attacks, which appear to be due to some endemic cause. At the time they became so severe, Napoleon was within two months of the date of his death, and, as we know, the cancer was in a state of ulceration, and involved nearly the whole body of the organ. Now most writers on the subject say that the terminal stages of gastric cancer are apt to be attended with an elevation of the temperature.

Fenwick has dealt with this point, and maintains that a third of the cases of gastric cancer exhibit rises of temperature. He also says that fifty-eight per cent. of those cases in which the body of the stomach is involved show pyrexia. He describes the rise of temperature as being accompanied by chills, rigors, headaches, pains in the limbs, and the defervescence as being attended with profuse sweatings. It is, therefore, an interesting problem to determine how much of these febrile manifestations was due to the septic absorption going on from the ulcerated cancer, and how much to the endemic form of fever with which Napoleon was supposed to be affected.

But although this question may be debated when dealing with the later stages of the illness, it can afford no explanation of the same symptoms exhibited during the earlier stages, and there appears to be no other solution than the one offered by Professor Keith, namely, that Napoleon was affected with some form of endemic fever. In connection with this the early history of Napoleon's life is most important, and Mr. Norwood Young, in his able and thoughtful book, *The Growth of Napoleon*, has demonstrated the fact that Napoleon when a young man was seriously attacked with "ague." In a letter quoted under date 1787 Napoleon says: "I

myself have been tormented for a month past by a tertian fever"; and during the greater part of a vacation which he was spending in Corsica at the time, he was apparently in poor health. Again, when at Auxonne in 1789, he writes: "This neighbourhood is very unhealthy by reason of the adjoining marshes . . . I have had for certain periods of time a continuous fever, which left me four days' repose, and then returned. It has weakened me, and has given me long periods of delirium." So in early life Napoleon was affected with some form of malaria for a period of more than a year. Speaking of Ajaccio, Mr. Young mentions that malaria is prevalent, and that in summer the inhabitants migrate to the hills. There is, therefore, a clear history of infection of an endemic kind, prevalent in the islands of the Mediterranean; and it is by no means impossible that the seeds thus sown may have flourished again when Napoleon went to reside in the sub-tropics of St. Helena.

Later in the career of the Emperor, another account has been given of an attack of illness which bore some resemblance to malaria. Ségur mentions in his classic, *La Campagne de Russie*, that Napoleon on the eve of the battle of Borodino was seized with an attack of fever, which lasted on and off for five days, and considerably interfered with his dispositions at that time.

In order to understand this question, attention must be paid to the climatic conditions subsisting in St. Helena, and the effect they had on those living there during the period of captivity. In few matters connected with this subject have such contradictory views been expressed, and in order to place before you the true position, I have made a study of the health returns of the regiments quartered in St. Helena at that time, and have also inquired into the state of health amongst those living at Longwood.

First, as to Longwood: Napoleon's household consisted of from forty to fifty people, and during the whole period of the captivity but two deaths are recorded, those of Napoleon and Cipriani. Cipriani, the *maître d'hôtel*, was suddenly seized with abdominal pain, and died, after five days' illness, with all the symptoms of general acute peritonitis. From time to time some of the inmates of Longwood were indisposed, and Gourgaud in particular had a sharp attack of dysentery soon after his arrival in St. Helena. But there does not appear to be any record of people at Longwood being attacked with symptoms similar to those from which the Emperor suffered. Then if, as it would be reasonable to infer, the milk and the water were sources of infection, it must in fairness be stated that Napoleon drank neither milk nor water except boiled with

his coffee. It must also be remembered that the household at Longwood was strictly confined within limits, and its members rarely went outside those limits except for short periods. Therefore, if sources of infection existed at Longwood, one would expect to find evidences of it in a circumscribed community, but although there is no indication of this, yet it is possible that some of the indisposition mentioned from time to time may have been due to this cause.

The health of the island generally, and particularly the health of the regiments quartered there, has an important bearing on this subject. The best guide to the health of the island is to be found in the monthly returns of sickness and deaths amongst the troops stationed in St. Helena. Now it is an extraordinary fact that during the whole period of the captivity no officer died on the island, with the exceptions of Lieutenants Davy and Macdougall of the 66th, who were drowned while fishing, and the proportion of those who were granted sick leave is extremely small. In fact, the officers enjoyed excellent health, and their indisposition was as a rule of a trifling kind. When, however, the sickness and deaths of the rank and file are investigated, an appalling state of affairs is disclosed. I will not weary you with statistics, for they can be consulted in the Appendices II

and III at the end of this paper, but I will epitomize the results of my investigations.

The average strength of the Foot Regiments quartered in St. Helena during the six years of the captivity was 1096, and every month there was an average of 71 men on the sick-list. The total number of deaths during the same period was 256, and the annual death-rate was, therefore, approximately 40 per thousand. In the 66th Regiment it rose as high as 63 per thousand, and was never below 23 per thousand, as in the case of the 53rd Regiment. It has been contended frequently that the high rate of illness amongst the troops was noticeable only when they were quartered at Jamestown and adjoining camps, places which were admittedly unhealthy; but my investigations disclose no great difference between the mortality returns when the troops were stationed at Jamestown, and when quartered at Deadwood, within a mile of Longwood. How Surgeon Henry, in his *Events of a Military Life*, can assume that the sickness was not exceptional in the face of these facts passes all comprehension.

It has also been asserted that the high rate of mortality in the 66th was on account of the fact that the regiment had come straight from India, and was therefore in a debilitated condition. The mortality was 63 per thousand. But the

20th, another regiment in St. Helena, although coming straight from England, and although stationed at Deadwood, a healthy place, had an annual mortality of 53 per thousand. In order to be quite certain I have made a comparison between the regiments stationed respectively in St. Helena, India, and England. During the period under review, the annual death-rate was 65 per thousand for India, 16 per thousand for England, and 40 per thousand for St. Helena. It seems impossible, therefore, in face of these facts, to resist the conclusion that, for rank and file at any rate, St. Helena was an unhealthy climate. The diseases responsible for this high death-rate in St. Helena were: (1) dysentery and bowel complaints; (2) liver complaints; (3) effects of the sun; and (4) chest complaints.

There is much, therefore, to be said for Professor Keith's "endemic theory"; and from a consideration of the evidence it appears to offer the only possible explanation of one set of symptoms which Napoleon exhibited.

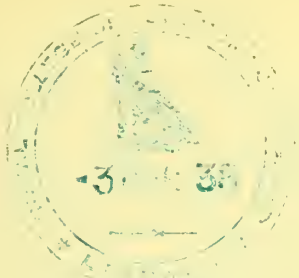
The evidence regarding the malady of Napoleon has now been placed before you, I trust without prejudice, and without any attempt to press any view unduly. I hope you are now in a position to answer the questions which I ventured to ask at the beginning of this paper, and which I repeat again :—

1. What were the diseases from which Napoleon suffered?

2. What were the probable causes of these maladies?

3. How far did the results of the post-mortem examination substantiate the clinical evidence of those diseases?

In the preparation of this paper the usual sources of information have been freely used and consulted, but particular mention may be made of the *Lowe Papers* in the British Museum, and the "Monthly Returns" of Sickness and Deaths in Regiments in the Record Office. Much valuable matter has also been obtained from Mr. G. L. de St. M. Watson, and his book, *A Polish Exile with Napoleon*, has been of the greatest use. To him and to Mr. Norwood Young, who has been most helpful with criticism and information, my sincere thanks are due.



APPENDIX I

The Post-mortem Examination of Napoleon Bonaparte.—The evidence in existence concerning the state of the Liver

ANTOMMARCHI'S ACCOUNT

"The spleen, and the liver which was hardened, were very large and distended with blood.

The tissue of the liver, which was reddy-brown in colour, did not, however, present any other notable alteration in structure.

The liver, which was affected with chronic hepatitis, was closely united by its convex surface to the diaphragm; the adhesion extended over the whole organ, and was strong, cellular, and of long standing. The concave surface of the left lobe of the liver adhered closely and firmly to the corresponding part of the stomach, especially along the small curve of that organ, and also to the little epiploon.

At every point of contact the lobe was sensibly thickened, swollen and hardened."

HENRY'S ACCOUNT

(*Lowe Papers*, vol. 20,214, p. 200)

"No abscess, no hardness, no enlargement, no inflammation (of the liver) were observed. On the contrary, the liver was of natural size, and perfectly healthy in its internal parts.

There was a small adhesion of the convex surface of the left lobe to the diaphragm, which appeared to have been a continuation and a consequence of the adjoining adhesions between the liver and the stomach.

When the stomaeh was brought into view, an adhesion of great extent was pereeived between its superior surface and the eoneave surfaee of the left lobe of the liver."

THE OFFICIAL REPORT

(*Lowe Papers*, vol. 20,133)

" Strong adhesions eonneeted the whole superior surface (of the stomaeh), partieularly about the pyloric extremity, to the eoncave surfaee of the left lobe of the liver.

The eonvex surface of the left lobe of the liver adhered to the diaphragm, but with the exeeption of the adhesions oeeasioned by the disease of the stomaeh, no unhealthy appearanee presented itself in the liver."

In the original draft of the Offieial Report oeeurred the words: " The liver was perhaps a little larger than natural " (*Lowe Papers*, vol. 20,157, f. 20).

RUTLEDGE'S ACCOUNT

(*Lowe Papers*, vol. 20,133, f. 139)

" There was no adhesion of the liver to the diaphragm, exeepting through the medium of a little eoagulable lymph, whieh I easily removed with my finger when taking out the liver for examination. The part of the left lobe of the liver whieh had been in eontaet with the eancerated part of the stomaeh was indurated, and there was a superfieial thiekening whieh extended to about one-fourth of an inch round the eireumference of the eaneer. The remainder of the left lobe was free from disease."

SIR THOMAS READE'S ACCOUNT

(*Lowe Papers*, vol. 20,133, f. 133)

" The liver was afterwards examined. The moment the operator took it out, Dr. Shortt instantly observed, ' it was enlarged.' All the other medieal gentlemen differed with

him in this opinion, particularly Dr. Burton, who combated Dr. Shortt's opinion very earnestly. Dr. Henry was equally divided with Dr. Burton. Dr. Arnott said there was nothing extraordinary in the appearance of the liver; 'it might probably be a large one, but certainly not larger than the liver of any man of the same age as General Bonaparte.' Dr. Mitchell said he saw nothing extraordinary, and Mr. Rutledge said it certainly was not enlarged, notwithstanding all these observations.

Dr. Shortt still persisted in saying 'it was enlarged.' This struck me so forcibly that I stepped forward and observed to the medical officers generally, that it appeared to me very important that they should all be prepared to give a decided and prompt opinion as to the real state of the liver, and I recommended a very careful examination of it. Dr. Shortt made no more observations, but all the other gentlemen reiterated their first opinion to me. At this moment the liver was in the hands of the operator, and upon my appearing desirous to see it close, he took his knife and cut it from one end to the other, observing to me: 'It is good, perfectly sound, and nothing extraordinary in it.' He observed at the same time that he thought it was a large liver. His opinion, however, did not appear to have been made in the same manner as Dr. Shortt had expressed it, viz. 'that the liver was enlarged.' There is a wide difference between 'a large liver' and 'a liver being enlarged.' I made this observation to Dr. Burton and Dr. Arnott, who coincided."

ANTOMMARCHI'S STATEMENT, DR. BURTON'S LETTER

(*Lowe Papers*, vol. 20,214)

Dr. Shortt asked Antommarchi to add his signature to the official document giving the results of the post-mortem examination. Antommarchi replied that he agreed perfectly with the British medical officers, but that as the document was written in a language which he did not under-

stand, it might appear strange if he annexed his signature to it. Dr Shortt then offered to translate it into Italian, and Dr. Burton offered to do the same for him in French; and as Count Bertrand understood English, the faithfulness of the translation could be verified. Antommarchi then had the Report translated, *with the correctness of which he expressed himself as quite satisfied.* He then asked Count Bertrand what he should do. The Count objected to Antommarchi signing the report on the *sole ground* that in it the deceased was not designated "the Emperor Napoleon."

DR. THOMAS SHORTT'S ACCOUNT

(From unpublished documents among the Shortt Papers)

"On opening the body every part of it was sound excepting the stomach, which was a perfect mass of disease from cancer and ulcerated in several places. In one place near the lower opening there was a hole sufficiently large to admit the little finger, which penetrated the coats of the stomach." (Letter to his father, dated May 7, 1821.)

The original draft of the Report of the post-mortem examination is in the possession of the Shortt family, in Dr. Shortt's handwriting, and in it, after the word "diaphragm," are the words: "The liver was perhaps a little larger than natural." This sentence is crossed out, and a footnote says: "The words crossed out were suppressed by the order of Sir Hudson Lowe. (Signed) Thomas Shortt, P.M.O."

APPENDIX II

Tables showing the Mortality in the Foot Regiments stationed in St. Helena

(From the "Monthly Returns" in the Record Office)

FIFTY-THIRD, SIXTY-SIXTH, AND TWENTIETH REGIMENTS

Collective Returns.

Period under review: April 1816 to March 1822 inclusive; 72 months, or six years.

Average strength of all Regiments . . .	1096
Deaths during the period under review . .	256
Deaths per annum	42
Death-rate per annum . . .	40 *per thousand*

Average number of sick men per month, 71.

FOR COMPARISON:

Regiments stationed in England.

Average death-rate per annum, 16 *per thousand.*

Regiments stationed in India.

Average death-rate per annum, 65 *per thousand.*

FIFTY-THIRD REGIMENT

(2nd Battalion).

Where stationed : Deadwood.

Period under review : April 1816 to June 1817 inclusive; 15 months.

Average strength of the Battalion	558
Deaths during the period under review	17
Deaths per annum	13
Death-rate per annum	23 *per thousand*

Average number of sick men per month, 38.

FOR COMPARISON:

Fifty-third Regiment

(2nd Battalion).

Where stationed : England.

Period under review : the year 1815.

Average strength	544
Deaths during the year	7
Death-rate per annum	11 *per thousand*

Average number of cases of sickness per month, 22.

FOR COMPARISON:

Fifty-third Regiment.

Where stationed : Bangalore, India.

Period under review : the year 1821.

Average strength	806
Deaths for the year	36
Death-rate per annum	44 *per thousand*

SIXTY-SIXTH REGIMENT

(2nd Battalion).

Where stationed : Jamestown, possibly Francis Plain.
Period under review : April 1816 to June 1817 inclusive; 15 months.

Average strength of the Battalion . .	603
Deaths during the period under review. .	48
Deaths per annum	38
Death-rate per annum . . .	63 *per thousand*

Average number of sick men per month, 51.

FOR COMPARISON:

SIXTY-SIXTH REGIMENT

(1st Battalion).

Where stationed : North of England.
Period under review : the year 1822.

Average strength	483
Deaths during the year	4
Death-rate per annum . . .	8 *per thousand*

Average number of cases of sickness per month, 24.

FOR COMPARISON:

SIXTY-SIXTH REGIMENT

(1st Battalion).

Where stationed : Dinapore, India.
Period under review : the year 1816.

Average strength	886
Deaths for the year	102
Death-rate per annum . .	115 *per thousand*

N.B.—During this year 35 men died of Cholera, if these are deducted the death-rate per annum is 75 *per thousand.*

SIXTY-SIXTH REGIMENT

(1st and 2nd Battalions).

Where stationed : Deadwood, Jamestown and Francis Plain.
Period under review : July 1817 to February 1820 inclusive 32 months.

Average strength of the two Battalions	956
Deaths during the period under review	93
Deaths per annum	36
Death-rate per annum	30 *per thousand*

Average number of sick men per month, 66.

FOR COMPARISON :

THIRTEENTH REGIMENT.

Where stationed : Jersey.
Period under review : the year 1817.

Average strength	627
Deaths during the year	12
Death-rate per annum	19 *per thousand*

Average number of cases of sickness per month, 40.

SIXTY-SIXTH REGIMENT

(1st Battalion).

Where stationed : Jamestown and Francis Plain.
Period under review : March 1820 to May 1821 inclusive ; 15 months.

Average strength of the Battalion	622
Deaths during the period under review	23
Deaths per annum	18
Death-rate per annum	30 *per thousand*

Average number of sick men per month, 34.

FOR COMPARISON:

TENTH REGIMENT.

Where stationed: England.
Period under review: the year 1823.

Average strength	501
Deaths for the year	12
Death-rate per annum	23 *per thousand*

FOR COMPARISON:

SIXTY-SEVENTH REGIMENT.

Where stationed: Meerut, India.
Period under review: the year 1816.

Average strength	888
Deaths during the year	42
Death-rate per annum	47 *per thousand*

TWENTIETH REGIMENT

Where Stationed: Jamestown, Francis Plain, Lemon Valley.
Period under review: May 1819 to February 1820 inclusive; 10 months.

Average strength of the Regiment	595
Deaths during the period under review.	27
Deaths per annum	32
Death-rate per annum	53 *per thousand*

Average number of sick men per month, 43.

FOR COMPARISON:

TWENTY-FOURTH REGIMENT.

Where stationed: Dinapore, Calcutta.
Period under review: the year 1817.

Average strength	896
Deaths during the year	62
Death-rate per annum	61 *per thousand*

TWENTIETH REGIMENT

Where stationed : Deadwood.
Period under review : March 1820 to May 1821 inclusive; 15 months.

Average strength of the Regiment	598
Deaths during the period under review	35
Deaths per annum	28
Death-rate per annum	50 *per thousand*

Average number of siek men per month, 33.

FOR COMPARISON:

Sixty-fifth Regiment.

Where stationed : Bombay.
Period under review : the year 1817.

Average strength	546
Deaths during the year	22
Death-rate per annum	40 *per thousand*

TWENTIETH REGIMENT

Where stationed : Franeis Plain and Jamestown.
Period under review : June 1821 to Mareh 1822 inelusive; 10 months.

Average strength of the Regiment	537
Deaths during the period under review	9
Deaths per annum	*approximately* 11
Death-rate per annum	20 *per thousand*

Average number of siek men per month, 23.

FOR COMPARISON:

Twentieth Regiment.

Where stationed : Bombay.
Period under review : the year 1823.

Average strength	682
Deaths for the year	45
Death-rate per annum	66 *per thousand*

APPENDIX III

Proportion of Sick Days amongst the Foot Regiments stationed in St. Helena

From September 25, 1815, to December 31, 1821.

(Record Office, Muster Rolls, W. O. 12)

FIFTY-THIRD, SIXTY-SIXTH, AND TWENTIETH REGIMENTS

Collective Returns.

The period under review, in days, was 2287.
The average strength on the island of these three Regiments was 1096.
The total number of " Troop Days " was, therefore, 2,506,552.
The total number of " Sick Days " was 121,203.
Therefore, the percentage of " Sick Days " was 4·75 (*approx.*).
Average number of " Sick Days " per man was 110.

FIFTY-THIRD REGIMENT

(2nd Battalion).

Where stationed : Deadwood.

Period under review : September 15, 1815, to June 24, 1817, or in days, 637.
Average strength, 558.
The total number of " Troop Days " was, therefore, 355,446.
The total number of " Sick Days " was 13,284.
Therefore, the percentage of " Sick Days " was 3·75.
Average number of " Sick Days " per man was 24.

SIXTY-SIXTH REGIMENT
(1st Battalion).

Where stationed : Francis Plain and Deadwood.

Period under Review : July 25, 1817, to March 24, 1821, or in days, 1284.
Average strength, 622.
The total number of " Troop Days " was, therefore, 896,648.
The total number of " Sick Days " was 56,182.
Therefore, the percentage of " Sick Days " was 6·25 (*approx.*).
The average number of " Sick Days " per man was 91.

SIXTY-SIXTH REGIMENT
(2nd Battalion).

Where stationed : Jamestown.

Period under review : April 17, 1816, to July 24, 1817, or in days, 463.
Average strength, 603.
The total number of " Troop Days " was, therefore, 279,189.
The total number of " Sick Days " was 19,209.
Therefore, the percentage of " Sick Days " was 7 (*approx.*).
The average number of " Sick Days " per man was 32.

TWENTIETH REGIMENT

Where stationed : Jamestown, Francis Plain, Deadwood.

Period under Review : March 24, 1819, to December 31, 1821, or in days, 1012.
Average strength, 595.
Total number of " Troop Days " was, therefore, 602,140.
Total number of " Sick Days " was 32,528.
Therefore, the percentage of " Sick Days " was 5·25 (*approx.*).
Average number of " Sick Days " per man was 54.

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